SOME ACCOUNT
OF THE
DISCOVERY,
MADE BY THE LATE
MR. JOHN DOLLOND, F.R.S.
WHICH LED TO THE GRAND IMPROVEMENT OF
REFRACTING TELESCOPES,
IN ORDER TO CORRECT SOME MISREPRESENTATIONS,
IN FOREIGN PUBLICATIONS, OF THAT DISCOVERY:

WITH
An ATTEMPT to account for the MISTAKE in an Experiment
made by SIR ISAAC NEWTON;

ON WHICH EXPERIMENT, THE IMPROVEMENT OF THE
REFRACTING TELESCOPE ENTIRELY DEPENDED.

By PETER DOLLOND,
MEMBER OF THE AMERICAN PHILOSOPHICAL SOCIETY AT
PHILADELPHIA.

LONDON:
PRINTED FOR J. JOHNSON, IN ST. PAUL'S CHURCH-YARD.
M.DCC.LXXXIX.

Jos: Banks

ADVERTISEMENT.

MY Intention in writing the following Paper was, to correct ſeveral falſe Repreſentations, relating to the Invention of the Achromatic Refracting Teleſcope, and to ſecure to my late Father, Mr. John Dollond, as well as to this Country, the Honour of ſo valuable a Diſcovery.

With this View, the Paper was preſented to the Royal Society, by the Reverend Dr. Maſkelyne, Aſtronomer Royal, in Expectation of its being publiſhed in the Philoſophical Tranſactions. It was read at a Meeting of the Society on the 21ſt of

May, 1789; but afterwards, contrary to my Expectation, it was reſolved, in a Council of the Society, that the Paper ſhould *not* be printed in their Tranſactions; I therefore take this Method of ſubmitting it to the Public; as I humbly conceive, it relates to a Subject of a ſufficient Degree of Importance to claim their Attention.

PETER DOLLOND.

Some

Some Account of the Diſcovery, made by the late Mr. John Dollond, F.R.S. which led to the Grand Improvement of Refracting Teleſcopes, in order to correct ſome Miſrepreſentations in Foreign Publications of that Diſcovery: With an attempt to account for the Miſtake in an Experiment made by Sir Iſaac Newton; on which Experiment the Improvement of the Refracting Teleſcope entirely depended. By Mr. Peter Dollond, Member of the American Philoſophical Society at Philadelphia.

THE correction of any inaccuracies or falſe repreſentations in the hiſtory of ſcience is certainly of ſome conſequence to the Public, and deſerves the attention of the Royal Society; particularly ſo, when ſuch falſe repreſentation tends to deprive any one of that praiſe, to which he may be juſtly entitled, by having contributed towards the advancement of ſcience; even though it may be in things of little moment. Then certainly it muſt be much more ſo, when it relates to matters of great importance; ſuch as was the diſcovery which brought forward the grand improvement of the Refracting Teleſcope.

I was led to theſe reflections, by having ſeen ſome accounts of that diſcovery in different publications, which were related in a manner that leſſened the merit of my late father John Dollond, and gave it to others, who never thought themſelves in any manner entitled to claim with him, or ever appeared to be inclined ſo to do. Their own characters were too exalted in ſcience to need the additional merit of any diſcovery, to which they had not an undoubted right.

The celebrated M. Euler, of Berlin, and M. Klinginſtierna, Profeſſor of Mathematics at Upſal, in Sweden, are the perſons alluded to. Theſe gentlemen have been mentioned by different Foreign Authors, who have had occaſion to give accounts of the improvement of the Refracting Teleſcope, as being the diſcoverers of the PRINCIPLE on which that improvement was founded; and nothing has been left to Dollond, but the credit of being the firſt who put the ſame into practice; whereby he has been deprived of the honour which is juſtly due to his memory, for having made ſo uſeful a diſcovery.

In order to ſet this matter in a proper light, I ſhall mention ſo much from Sir Iſaac Newton's Optics, as is neceſſary for the purpoſe; and then endeavour to prove, that what was attempted by Euler and Klinginſtierna was not done from any knowledge of the principle on which the improvement was founded; but that Dollond was actually the diſcoverer of that Principle, as well as the perſon who firſt put the ſame in practice.

When Newton had made his great diſcovery of the different refrangibility of light, he fully explained *that* to be the cauſe of the imperfection of Refracting Teleſcopes, and that it was not occaſioned by the ſpherical figures of the glaſſes, as had been the generally received opinion. But as mathematicians had made many attempts to correct the errors ariſing from the ſpherical figures, by giving the glaſſes figures from the conic ſections, he took that opportunity of mentioning an ingenious thought of his own, of compoſing the object-glaſſes of two glaſſes with water between them; by which means he ſays, That the ſpherical figures of the glaſſes might have been corrected, and teleſcopes brought to a ſufficient perfection, had it not been for the different refrangibility of the ſeveral ſorts of rays.—*Newton's Op. 3d. Edition, p.* 90.

Newton having compleated the principal experiments relating to the different refrangibility of light, and having determined the proportions of the ſines of incidence to the ſines of refraction in the different coloured rays, as given by his glaſs priſms, proceeds to try the eighth experiment of the ſecond part of the firſt book of his Optics, to diſcover their proportions in different refracting mediums. This experiment he tried, by placing a priſm of glaſs in a priſmatic veſſel of water. Refracting the light through theſe different mediums, he found that light, as often as by contrary refractions it is ſo corrected that it emergeth in lines parallel to thoſe in which it was incident, continues ever after to be white; but if the emergent rays be inclined to the incident, the light will become coloured.—*Newton's Optics, p.* 112.

The concluſion drawn from this experiment was, that the divergency of the different coloured rays was conſtantly in a given proportion to the mean refraction in all ſorts of refracting mediums. This was the Principle eſtabliſhed by the Newtonian experiment, and was doubted by no one, until the beginning of the year 1757; when Dollond tried the ſame experiment as above related, and found the reſult to be very different; for the light after being refracted in contrary directions through the glaſs and water priſms, if the emergent rays were parallel to the incident rays, they were found to be conſiderably coloured; from whence it followed, that the diſſipation of the different coloured rays was not in the ſame proportion to the mean refraction in water as in glaſs. And further experiments proved, that there was alſo a very conſiderable difference of the ſame nature to be found in different kinds of glaſs.—*See Dollond's account, Phil. Tranſ. vol. l. p.* 743.

This was the New Principle, which brought forward the improvement of Refracting Teleſcopes; a principle ſo contrary to the generally received opinion, that Euler had much difficulty to prevail on himſelf to believe what was told him by his friends on that ſubject; as appears by his own papers publiſhed in the Memoirs of the Royal Academy at Berlin. For he firſt ſuppoſes the goodneſs of Dollond's Teleſcopes to be owing to the greeniſh colour of the crown-glaſs, which did not tranſmit all the red rays; he afterwards endeavours to account for it from the conſtruction of the eye-glaſſes; and at laſt declares it to be very extraordinary, that the Engliſh Optician ſhould have made ſuch an improvement, by reaſoning, as it were, in a manner quite contrary to the nature of things; for ſo indeed the new diſcovered principle appeared to him. Notwithſtanding theſe declarations of Euler, which were publiſhed in the year 1762, M. De la Lande, in the ſecond volume of his Aſtronomy, p. 837, publiſhed in the year 1764, ſays, That Euler, in 1747, endeavoured to correct the different refrangibility of light, by a method which Newton pointed out for correcting the errors of the ſpherical figures of the glaſſes; which was, by two lenſes with water between them, as recited above: and he ſays, That Dollond tried to confute Euler, who had demonſtrated an error in Newton's theory of colours; but the diſpute having given occaſion to Dollond to examine the thing more narrowly, he afterwards acknowledged the error of Newton, and in the year 1759 he found out a method of making achromatic teleſcopes that ſucceeded very well.

Now this account of De la Lande's is by no means the true ſtate of the facts, as appears by the letters which paſſed between Euler and Dollond, publiſhed in Phil. Tranſ. vol. xlviii. p. 287; for though Euler argues from his hypotheſis, that the reſult of Newton's experiment could not be exactly as he relates it, yet he does not pretend to controvert any of Newton's laws of refraction, as being contrary to experiment, but believed, that the divergency of the different coloured rays differed ſcarce ſenſibly from bearing a given proportion to the mean refraction, in all ſorts of refracting mediums; by which it appears, that the error afterwards diſcovered by Dollond was not even ſuſpected by Euler; therefore that part of De la Lande's account cannot be true; for Dollond could not be ſaid to acknowledge an error, ſuppoſed to be diſcovered by Euler in Newton's theory of colours, by having actually diſcovered one himſelf of a different nature. The true ſtate of the fact is, That in 1747 Euler endeavoured to correct the errors ariſing from the different refrangibility of light in object-glaſſes, by a method which was not founded on any experiment, but on an hypotheſis, which did not appear to be on a true principle, ſo that the attempts which were made to put this method in practice did not ſucceed: this was certainly the caſe; for after M. Clairaut had examined the controverſy between Euler and Dollond, he pronounced, that rections through the glaſs and water priſms, if the emergent rays were parallel to the incident rays, they were found to be conſiderably coloured; from whence it followed, that the diſſipation of the different coloured rays was not in the ſame proportion to the mean refraction in water as in glaſs. And further experiments proved, that there was alſo a very conſiderable difference of the ſame nature to be found in different kinds of glaſs.—*See Dollond's account, Phil. Tranſ. vol. l. p.* 743.

This was the New Principle, which brought forward the improvement of Refracting Teleſcopes; a principle ſo contrary to the generally received opinion, that Euler had much difficulty to prevail on himſelf to believe what was told him by his friends on that ſubject; as appears by his own papers publiſhed in the Memoirs of the Royal Academy at Berlin. For he firſt ſuppoſes the goodneſs of Dollond's Teleſcopes to be owing to the greeniſh colour of the crown-glaſs, which did not tranſmit all the red rays; he afterwards endeavours to account for it from the conſtruction of the eye-glaſſes; and at laſt declares it to be very extraordinary, that the Engliſh Optician ſhould have made ſuch an improvement, by reaſoning, as it were, in a manner quite contrary to the nature of things; for ſo indeed the new diſcovered principle appeared to him. Notwithſtanding theſe declarations of Euler, which were publiſhed in the year 1762, M. De la Lande, in the ſecond volume of his Aſtronomy, p. 837, publiſhed in the year 1764, ſays, That Euler, in 1747, endeavoured to correct the different refrangibility of light, by a method which Newton pointed out for correcting the errors of the ſpherical figures of the glaſſes; which was, by two lenſes with water between them, as recited above: and he ſays, That Dollond tried to confute Euler, who had demonſtrated an error in Newton's theory of colours; but the diſpute having given occaſion to Dollond to examine the thing more narrowly, he afterwards acknowledged the error of Newton, and in the year 1759 he found out a method of making achromatic teleſcopes that ſucceeded very well.

Now this account of De la Lande's is by no means the true ſtate of the facts, as appears by the letters which paſſed between Euler and Dollond, publiſhed in Phil. Tranſ. vol. xlviii. p. 287; for though Euler argues from his hypotheſis, that the reſult of Newton's experiment could not be exactly as he relates it, yet he does not pretend to controvert any of Newton's laws of refraction, as being contrary to experiment, but believed, that the divergency of the different coloured rays differed ſcarce ſenſibly from bearing a given proportion to the mean refraction, in all ſorts of refracting mediums; by which it appears, that the error afterwards diſcovered by Dollond was not even ſuſpected by Euler; therefore that part of De la Lande's account cannot be true; for Dollond could not be ſaid to acknowledge an error, ſuppoſed to be diſcovered by Euler in Newton's theory of colours, by having actually diſcovered one himſelf of a different nature. The true ſtate of the fact is, That in 1747 Euler endeavoured to correct the errors ariſing from the different refrangibility of light in object-glaſſes, by a method which was not founded on any experiment, but on an hypotheſis, which did not appear to be on a true principle, ſo that the attempts which were made to put this method in practice did not ſucceed: this was certainly the caſe; for after M. Clairaut had examined the controverſy between Euler and Dollond, he pronounced, that what Euler had done appeared to be more ingenious than uſeful.

Euler indeed ſays, that the ſtructure of the eye gave him the greateſt reaſon to ſuppoſe, that the different refrangibility might be corrected by ſeveral refractions through different kinds of mediums; for which purpoſe he thought the eye to be ſo conſtructed. But this reaſoning had no weight with Dollond, as he perceived and mentioned to his friends, That the refractions of the eye, at the ſeveral ſurfaces and humours, are all made the ſame way, and conſequently, for want of contrary refractions, the colours produced by the firſt refractions could not be taken away. How this can ſubſiſt with the perfection of our viſion, has been ingeniouſly explained by the Aſtronomer Royal, in an account which he propoſes to lay before this Society *.

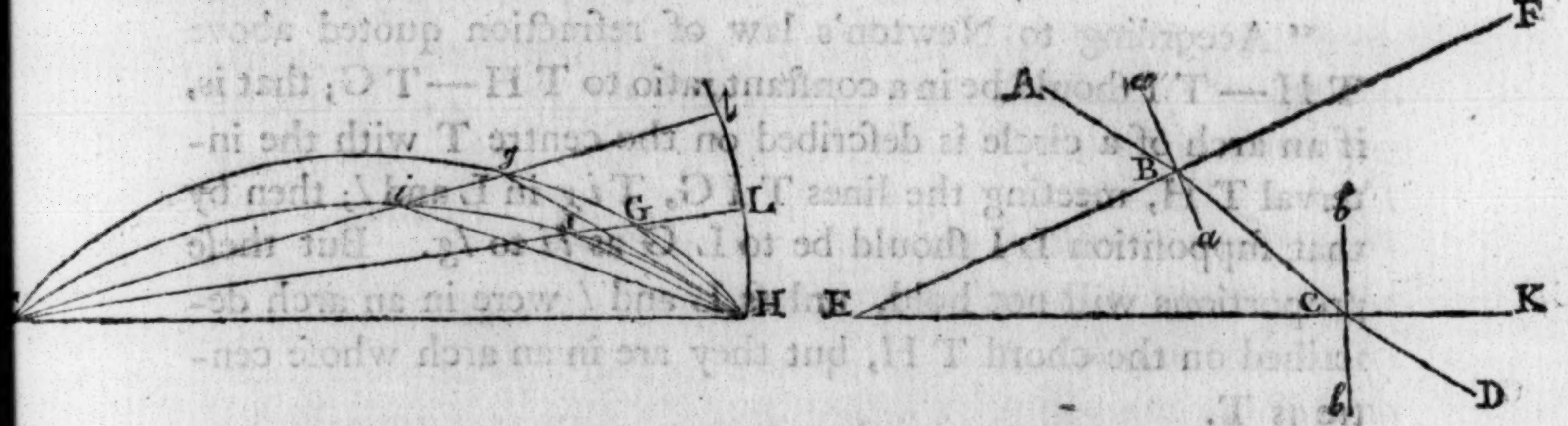
Klinginſtierna has likewiſe been conſidered as a party concerned in the improvement of the Refracting Teleſcope; though De la Lande does not mention his name, yet ſome others do. This has been occaſioned by his having, in the year 1755, conſidered the controverſy between Euler and Dollond, and having formed a theorem of his own, by which he was alſo induced to believe, that the reſult of Newton's experiment could not be as he had related it; except when the angles of the refracting mediums were ſmall. This he communicated to Dollond, in a letter to his friend Mr. Mallet, who was then in England. As this theorem has never been publiſhed in Engliſh, I ſhall give a copy of it here, as

---

* This account has been ſince read at the Royal Society, and will appear in the next publication of the Philoſophical Tranſactions.

taken by my father from Klinginſtierna's letter to Mallet, that mathematicians may judge of the truth of the deductions.

“ *Remarks on the Law of Refraction of Rays of Light of different Kinds, through different mediums. See Newton's Optics, Book* I. *Part* II. *Prop.* 3*d. Exper.* 8*th.*



“ Upon any right line, as T H, let there be drawn two arches T I H, T G H, and let a right line T I G be drawn interſecting the arches in I and G. Join I H and G H; let F E K be a tranſparent wedge, having its acute angle F E K equal to the angle I H G; let the two faces of this wedge be contiguous to two different tranſparent mediums; and let the ratio of refraction out of the medium, that joins the ſurface E F into the wedge, be as the ratio of T H to T I, and the ratio of refraction out of the wedge into the medium joining the ſurface E K as the ratio of T G to T H.

“ Now if AB repreſents a ray of light entering into the wedge, and the angle A B *a* is made equal to H I G, then will the angle C B *a* be equal to the angle T H I. Ang. B C *b* = ang. T H G and D C *b* = H G L; ſo that the incident ray A B will be parallel to the emergent ray C D, which has been twice refracted.

“ Now if the incident ray is compounded of divers ſimple rays, each of which, after two refractions, ſhould emerge parallel to the common incident ray, the refractions of each will be repreſented by ſo many right lines T *ig* joining H *i*, H *g*, the ſame as before.

“ According to Newton's law of refraction quoted above T H — T I ſhould be in a conſtant ratio to T H — T G; that is, if an arch of a circle is deſcribed on the centre T with the interval T H, meeting the lines T I G, T *ig* in L and *l*; then by that ſuppoſition L I ſhould be to L G as *li* to *lg*. But theſe proportions will not hold, unleſs L and *l* were in an arch deſcribed on the chord T H, but they are in an arch whoſe centre is T.

“ Therefore Newton's law of refraction does not ſeem to follow clearly from his 8th experiment, which our wedge with two contiguous mediums refers to.

“ If we ſhould ſuppoſe ſuch a law of refraction as we find neceſſary to bring out every ſimple ray parallel to the incident, after two refractions through this wedge F E K, it can be demonſtrated, that the ſame law will not have the ſame effect in another wedge of a different angle, but for every different angle there will be a different law required.

“ Whence it ſeems to follow, that there muſt be ſome miſtake in this experiment of Newton's, which he himſelf gives as an univerſal one, for it does not ſeem likely that the law which really obtains in nature ſhould depend upon a greater or leſs angle of a wedge.

"Nevertheleſs it muſt be obſerved, that the leſs the refractions are, the nigher will the Newtonian law be to that which is required for producing a perfect parallelism of the emergent rays to the common incident ray; for in this caſe L I to L G will be very nearly in a given ratio. It does not ſeem that the aberration of the rays in object-glaſſes, proceeding from the different refrangibility, can be corrected by any refractions, which is what Mr. Newton plainly inſiſts upon. However, this whole affair deſerves to be more accurately examined by experiments."

I ſhall here only remark, that it appears by this copy of a letter from Klinginſtierna, that the ſuppoſed error in the reſult of Newton's experiment, which he thus labours to demonſtrate, is the ſame as before attempted to be aſcertained by Euler, and not the error which was afterwards diſcovered by Dollond.

The account given by De la Lande, of the improvement of the Refracting Teleſcope, was copied by moſt Foreign Writers on the ſubject, with little variation, except in giving ſometimes a little more of the honour to Euler, and alſo making mention of Klinginſtierna.

But in the Eulogy on Euler, written and publiſhed by M. N. Fuſs, profeſſor of Mathematics at St. Peterſburg, in the year 1783, p. 41 and 42, he gives the whole of the diſcovery to Euler, except " that Dollond is allowed to have found out two ſorts of glaſs, which crowned at laſt, in 1757, the happy conjecture of Euler, by the invention of Achromatic Teleſcopes, which formed a new epoch in Aſtronomy and Dioptrics." As this account is the moſt curious of any I have

found,

ſound, I ſhall here give it at length, and contraſt it with what Euler ſays himſelf on the ſubject, in a paper read before the Royal Academy of Sciences at Berlin, in 1764, and publiſhed in the volume of their Memoirs for 1766, p. 119.

Mr. Fuſs ſays, "The examination of the Newtonian Theory had given Euler an opportunity of inveſtigating the different refrangibility of light, and the bad effects which the diſperſion of the colours produced in Refracting Teleſcopes, which had been almoſt entirely abandoned upon account of this defect. The conſideration of the wonderful ſtructure of the eye made him ſuppoſe, that a certain combination of different tranſparent bodies could remedy this inconvenience. He propoſed for this purpoſe, in the year 1747, object-glaſſes compoſed of two glaſſes, the cavity between which could be filled with water.

"His opinion was attacked by the famous Engliſh Artiſt, Dollond, who oppoſed to him the authority of Newton: M. Euler ſoon ſhewed him the error of his principles. Some experiments made upon meniſcuſes, the cavities of which were filled with different liquids, confirmed him in his opinion: and Mr. Dollond, who had in the mean time diſcovered two ſorts of glaſs, which were proper for examining it further, crowned at laſt, in 1757, the happy conjecture of M. Euler, by the invention of Achromatic Teleſcopes, which formed an epoch in Aſtronomy and Dioptrics.

"The ſucceſs of Mr. Dollond, who availed himſelf, with ſo much advantage, of a diſcovery, which he had at firſt attacked as contrary to experiment, induced M. Euler to extend his reſearches further upon the ſubject of Dioptric Inſtruments," &c. &c.

I ſhall now ſubjoin a tranſlation of M. Euler's paper read at the Academy of Sciences at Berlin, in the year 1764.

" Although I have already frequently diſcuſſed this ſubject, I ſee myſelf again obliged to reſume it, in conſequence of the aſtoniſhing diſcoveries which have been lately made upon the nature of glaſs, and its different kinds. I am not aſhamed frankly to avow, that the firſt accounts, which were publiſhed of it, appeared ſo ſuſpicious, and even ſo contrary to the beſt eſtabliſhed principles, that I could not prevail upon myſelf to give credit to them. That there ſhould be two kinds of glaſs, in which the refraction of the mean rays is nearly equal, whilſt that of the extremes differs moſt enormouſly, appeared to me to ſhock good ſenſe; and perhaps I ſhould never have ſubmitted to the proofs, which Mr. Dollond produced to ſupport this ſtrange phenomenon, if Mr. Clairaut, who muſt at firſt have been equally ſurprized at it, had not moſt poſitively aſſured me, that Dollond's experiments were but too well founded. But at length the experiments made at Peterſburg by Mr. Zeiher have effectually ſucceeded in removing my prejudice; that ingenious philoſopher having inconteſtably proved, that it is the lead, which is uſed in ſome compoſitions of glaſs, that produces in it that ſtrange quality of augmenting the diſperſion of the extreme rays, without changing ſenſibly the refraction of the mean; and by increaſing the quantity of lead in the compoſition of glaſs, he has been enabled to make glaſs, which produces a much greater diſperſion of the rays than the flint-glaſs of Dollond.

" Now I muſt entirely renounce this principle, which until now has appeared ſo well-founded, That the diſperſion of the extreme rays depends ſolely upon the refraction of the mean rays; and I am obliged to acknowledge, That the diſperſion depends principally upon the quality of the glaſs, without the mean refraction thereof being ſenſibly affected thereby."

As it appears by the above paper, that Euler was at laſt ſo convinced of the truth of the diſcovery made by Dollond, as to renounce his favourite hypotheſis, it muſt be inferred, that the account given of this matter by Fuſs is very far from being the true ſtate of the facts, and indeed ſo much ſo, as to be very inexcuſable, even in an eulogy.

There is another publication of a later date, which I ſhall take the liberty of mentioning, "*Extracts of the Obſervations made at the Royal Obſervatory at Paris, in the year* 1787, *by Count Caſſini*." In page 106 he gives an account of the improvement of the Refracting Teleſcope, by way of prelude to his deſcribing a method, propoſed by M. l'Abbé Rochon, of putting fluids, and alſo a kind of maſtic, between the glaſſes of achromatic object-glaſſes, as a good method of mending bad glaſſes, or, as he calls it, a method to correct the *non-ſphericity* of the glaſſes; which he mentions as being ſimilar to that ingenious idea propoſed by Newton, for correcting the errors of the ſpherical figures of object-glaſſes. The account he gives of the improvement of the Refracting Teleſcope is as follows. He ſays, "It was the celebrated Euler who firſt propoſed to correct the errors ariſing from the different refrangibility, by uſing different refracting mediums, ſuch as water and glaſs. The late Mr. Dollond having availed himſelf of and realized this ingenious idea, has a juſt right to *partake* of the glory."—By theſe publications it ſeems, that Dollond, who explained the fallacy of Euler's hypotheſis, who afterwards diſcovered the True Principle, on which the different refrangibility of light could be corrected, and he, who put the ſame in practice, ſo much to the benefit of Science, is only to be allowed to *partake* of the glory, and that with Euler, who never himſelf thought he had the leaſt right to claim any part of the diſcovery with Dollond, as moſt fully appears by the paper above recited from the Berlin Memoirs.

I can account for theſe falſe repreſentations no other way than by ſuppoſing, that thoſe who wrote them have not taken ſufficient pains to inform themſelves of the true hiſtory of the diſcovery; for I would not wiſh to attribute what they have ſaid to any partiality; and I am induced to hope, that when the ſtate of facts which I have here adduced ſhall be candidly conſidered, that they will retract their declarations, and acknowledge, that Dollond was the ſole diſcoverer of the Principle which led to the improvement of Refracting Teleſcopes.

I now come to a more agreeable part of this paper, which is, to endeavour to reconcile the different reſults of the 8th experiment of the ſecond part of the 1ſt Book of Newton's Optics, as related by himſelf, and as it was found by Dollond, when he tried the ſame experiment, in the year 1757. Newton ſays, that light, as often as by contrary refractions it is ſo corrected, that it emergeth in lines parallel to the incident, continues ever after to be white. Now Dollond ſays, when he tried the ſame experiment, and made the mean refraction of the water equal to that of the glaſs priſm, ſo that the light emerged in lines parallel to the incident, he found the divergency of the light by the glaſs priſm to be nearly double to what it was by the water priſm. The light appeared to be ſo evidently coloured, that it was directly ſaid by ſome perſons, That if Newton had actually tried the experiment, he muſt have perceived it to have been ſo. Yet who could for a moment doubt the veracity of ſuch a character? Therefore different conjectures were made by different perſons. Mr. Murdoch in particular gave a paper to the Royal Society in defence of Newton; but it was ſuch as very little tended to clear up the matter. *Phil. Tranſ. vol.* liii. *p.* 192.—Some have ſuppoſed that Newton made uſe of water ſtrongly impregnated with *Saccharum Saturni*, becauſe he mentions ſometimes uſing ſuch water, to increaſe the refraction, when he uſed water priſms inſtead of glaſs priſms. *Newton's Opt. p.* 62.—And others have ſuppoſed, That he tried the experiment with ſo ſtrong a perſuaſion in his own mind, that the divergency of the colours was always in the ſame proportion to the mean refraction, in all ſorts of refracting mediums, that he did not attend ſo much to that experiment as he ought to have done, or as he uſually did. None of theſe ſuppoſitions having appeared at all ſatisfactory, I have therefore endeavoured to find out the true cauſe of the difference, and thereby ſhew, how the experiment may be made to agree with Newton's deſcription of it, and to get rid of thoſe doubts, which have hitherto remained to be cleared up.

It is well known, that in Newton's time the Engliſh were not the moſt famous for making optical inſtruments: Teleſcopes, Opera-glaſſes, &c. were imported from Italy in great numbers, and particularly from Venice; where they manufactured a kind of glaſs which was much more proper for optical purpoſes than any made in England at that time. The glaſs made at Venice was nearly of the ſame refractive quality as our crown-glaſs, but of a much better colour, being ſufficiently clear and tranſparent for the purpoſe of priſms. It is probable that Newton's priſms were made with this kind of glaſs; and it appears to be the more ſo, becauſe he mentions the ſpecific gravity of common glaſs to be to water as 2,58 to 1. *Newton's Opt. p.* 247, which nearly anſwers to the ſpecific gravity we find the Venetian glaſs generally to have. Having a very thick plate of this kind of glaſs, which was preſented to me about twenty-five years ago by the late Profeſſor Allemand, of Leyden, and which he then informed me had been made many years, I cut a piece from this plate of glaſs to form a priſm, which I conceived would be ſimilar to thoſe made uſe of by Newton himſelf. I have tried the Newtonian experiment with this priſm, and find it anſwers ſo nearly to what Newton relates, that the difference which remains may very eaſily be ſuppoſed to ariſe from any little difference which may and does often happen in the ſame kind of glaſs made at the ſame place at different times. Now the glaſs priſm made uſe of by Dollond to try the ſame experiment was made of Engliſh flint glaſs, the ſpecific gravity of which I have never known to be leſs than 3,22. This difference in the denſities of the priſms uſed by Newton and Dollond was ſufficient to cauſe all the difference which appeared to the two experimenters in trying the ſame experiment.

From this it appears, That Newton was accurate in this experiment as in all others, and that his not having diſcovered that, which was diſcovered by Dollond ſo many years afterwards, was owing entirely to accident; for if his priſm had been made of glaſs of a greater or leſs denſity, he would certainly have then made the diſcovery, and Refracting Teleſcopes would not have remained ſo long in their original imperfect ſtate.

FINIS.